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# Realism on the rocks: Novel success and James Hutton's theory of the earth



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## ABSTRACT

In this paper, I introduce a new historical case study into the scientific realism debate. During the late-eighteenth century, the Scottish natural philosopher James Hutton made two important successful novel predictions. The first concerned granitic veins intruding from granite masses into strata. The second concerned what geologists now term “angular unconformities”: older sections of strata overlain by younger sections, the two resting at different angles, the former typically more inclined than the latter. These predictions, I argue, are potentially problematic for selective scientific realism in that constituents of Hutton's theory that would not be considered even approximately true today played various roles in generating them. The aim here is not to provide a full philosophical analysis but to introduce the case into the debate by detailing the history and showing why, at least *prima facie*, it presents a problem for selective realism. First, I explicate Hutton's theory. I then give an account of Hutton's predictions and their confirmations. Next, I explain why these predictions are relevant to the realism debate. Finally, I consider which constituents of Hutton's theory are, according to current beliefs, true (or approximately true), which are not (even approximately) true, and which were responsible for these successes.

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## 1. Introduction

Perhaps the best known, most widely discussed argument for scientific realism is the “explanationist”, “abductive”, or “no-miracles” argument. According to this argument, the best – indeed, many would argue, the only – explanation for the empirical success of our best scientific theories is that they are true, or at least approximately true. If they are not, then this success would be some kind of “miracle” or “cosmic coincidence” (Maxwell, 1962, p. 18; Smart, 1963, p. 39; Putnam, 1975, p. 73; Brown, 1982; Boyd, 1989, pp. 7–9). An equally well known and extensively debated counterargument is that the history of science is replete with theories which, in their day, were highly successful, but which have turned out *not* to be (even approximately) true (Hesse, 1976, p. 264; Laudan, 1981).

This challenge from the history of science has undermined quite significantly the above inference from success to truth, forcing realists to modify their position in various ways. One strategy is to focus mainly on *novel predictive* success, since this is thought to provide greater warrant for realist commitment than other kinds of success (Musgrave, 1988; Lipton, 1990; Leplin, 1997, pp. 34–135).

Another widely adopted modification is to restrict that commitment to only those parts of theories that are/were “responsible for” their success. These, the realist argues, the “working” or “essentially contributing” parts, are (approximately) true. But the parts that are/were *not* responsible, that are/were merely “idle” or “presuppositional”, are not supported by the theory's success. There is no reason to believe that *they* are (approximately) true.

This view has been variously termed “*divide et impera* realism”, “deployment realism”, “selective realism” etc. – for present purposes I shall adopt the latter term. Versions of the position were first developed by Worrall (1989), Kitcher (1993, pp. 140–9), and Psillos (1994; 1999, pp. 96–139). More recent variations have been proposed by, among others, Harker (2013), Vickers (2013), and Peters (2014). The selective realist's version of the explanationist argument, then, is that the best – or only – explanation for the *novel predictive* success of our best scientific theories is that *those constituents of the theories that are responsible for the successful novel predictions* are (at least approximately) true. Just what kinds of constituents are responsible for such predictions, and precisely what this responsibility consists in, are very much open questions and subject to ongoing debate.

Following Vickers (2013, p. 190), Harker (2013, p. 98), and others (e.g., Psillos, 1999, pp. 105–6; Lyons, 2002, p. 70; Carrier, 2004, p. 148), I contend that the best way to assess selective realism is to

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look to the history of science for theories which are no longer considered (approximately) true but which were used to make successful novel predictions. We should examine these theories and their successes individually to determine whether the selective realist's strategy works in each case. To do this, we divide the theory in question into its various constituents. We then (a) consider which of these constituents are and are not, according to current theories,<sup>1</sup> (approximately) true, and (b) determine which constituents were responsible for the theory's success. If the responsible constituents are (approximately) true, then the case will lend support to selective realism. If, on the other hand, the responsible constituents are not (approximately) true, then the case will appear to constitute a "counterexample" to selective realism, rendering the position less plausible.

In this paper, I present a historical case which appears, at least *prima facie*, to constitute such a counterexample. During the late-eighteenth century, the Scottish natural philosopher James Hutton made two important successful novel predictions concerning the existence and characteristics of certain geological phenomena, namely, granitic veins and angular unconformities. Hutton made these predictions on the basis of a theory which, taken in its entirety, would not be regarded as (even approximately) true today. And constituents of Hutton's theory which by present lights are not (even approximately) true appear to have played important roles in his predictions.

The case is potentially very significant. As Saatsi (2012, p. 330) notes, more historical case studies from the special sciences are sorely needed in the realism debate. And although several cases from chemistry and the life sciences have been introduced, there are currently no serious cases from the history of geology being discussed in the literature. Introducing such a case is important, since we want to ensure that different formulations of realism apply equally well to different scientific disciplines, or at the very least we want to know whether we need different realisms for different sciences. To date, little has been said about geology in relation to realism at all. In his famous list of false-but-successful theories, Laudan (1981, pp. 121–2) includes "catastrophist geology". However, it is generally agreed that success in this case was not sufficiently novel to be pertinent to the debate. Elsewhere, Laudan (1984, p. 123) mentions the success that pre-1960s geology enjoyed despite its erroneous commitment to stable continents as a counterexample to realism. Against this, Kitcher (1993, p. 142) argues that the success of pre-1960s geology came in areas where the movement of continents was irrelevant, and that, therefore, the case is unproblematic and in fact supports a selective version of realism. This paper presents a challenge to Kitcher in that the movement of continents in the case of angular unconformities very plausibly is relevant.

The paper is divided into four main sections. First, I explicate Hutton's theory of the earth. I then give an account of Hutton's predictions and their confirmation. Following this, I explain why these predictions are relevant to the realism debate. Finally, I consider which constituents of Hutton's theory are and are not

(approximately) true, which constituents were responsible for the theory's success, and how the realist might respond to the case.

### 1.1. Hutton's theory, its formulation, and its constituents

According to Hutton's theory, the earth was divinely contrived for the sole purpose of providing a habitable world. A deist, Hutton believed that God designed the earth such that it would serve its purpose without any further intervention on His part. To this end, he thought, it was designed in a way analogous to an organic body in that it possesses a "reproductive" mechanism which enables it to maintain its purpose. In this system, matter is constantly eroded, washed into the sea, and deposited on the ocean floor. Sediments are then fused and consolidated by heat from subterranean molten matter and pressure from superincumbent sediment. Periodically, the hot, molten region becomes volatilised, causing it to expand, thereby elevating the strata to form new continents. These continents are then eroded, deposited, consolidated, and elevated to form yet more continents. The process is repeated indefinitely (see Hutton, 1785; 1788; 1795a; 1795b; 1899).

To better elucidate the roles they played in its success, it will be helpful to reconstruct the particular line of reasoning that led Hutton to formulate the various constituents of his theory. Like many Enlightenment thinkers, Hutton was greatly impressed by final causes. That of the earth, he believed, is evidently to provide a habitable world. Its motion, gravitational attraction to the sun, diurnal rotation, proportions of land, sea, and air, for example, are clearly calculated for the purpose of supporting life. That "the necessities of life" exist in such perfect measure, he emphasised further, attests to the infinite wisdom and beneficence of its Creator (Hutton, 1788, pp. 209–14, 216–7—quotation from p. 213; 1795a, pp. 3–13, 17–8).

A particular "necessary of life" with which Hutton was especially preoccupied was soil. Fertile soil, he noted, is essential for making a planet habitable. Soil, however, consists principally of fragments of rocks eroded by weather and transported down from higher regions to form fertile plains. It is then washed into the sea and replaced with more eroded matter. This matter, therefore, must inevitably become exhausted, reducing the earth to a great spheroid of water, unable to support life. The very process necessary to make the earth habitable, then, will eventually render it uninhabitable. He reasoned, however, that if the earth is divinely contrived, then it must possess some mechanism for replenishing the rocks such that they can continue to erode and supply fertile soil. To elucidate how such a restoration might be effected, he contended, we must consider the earth as analogous to an organic body. That is, we must think of it as possessing a reproductive system whereby the broken matter is continually repaired by the same forces responsible for its original formation (Hutton, 1788, pp. 214–6; 1795a, pp. 13–7).

To understand the restoration of land, then, Hutton proposed, we must consider how it was formed. He noted that the remains of marine animals in strata indicate that they formed in the ocean. They must therefore be composed of the same kinds of loose matter that we find on the ocean floor today, and which are evidently fragments of rocks eroded by weather and washed into the sea. This matter must somehow have been consolidated. For this to occur, it must first have been brought to a fluid state and then solidified. There are two possible ways this could be effected: (1) dissolution and crystallisation; or (2) heat and fusion. The former was insufficient, since many substances found in strata are water-insoluble. Heat, therefore, is the only possible cause of consolidation. It, unlike water, is capable of bringing all these substances to a fluid state. Sufficient pressure, moreover, supplied in this case by the weight of superincumbent sediment, will prevent the substances from

<sup>1</sup> This qualification is important. For while we cannot establish whether constituents of a given theory are (approximately) true, we can judge whether these constituents have been (approximately) retained in current theories. Since historical challenges to realism appeal to the success of past theories which are not considered (approximately) true in that they do not resemble current theories, all the realist needs to do to respond to such challenges is show that the responsible constituents of the theory in question are sufficiently like constituents of current theories. As is customary in the realism literature, I shall use the terms "approximately true" and "radically false" or "not even approximately true" to refer to constituents which have and have not been (approximately) retained in current theories respectively.

combusting or evaporating under such intense heat,<sup>2</sup> thereby enabling them to be fused and consolidated (Hutton, 1788, pp. 218–61; 1795a, pp. 18–120).

Following consolidation, the strata must have been either (a) elevated above the surface of the water, or (b) exposed by the ocean receding. A sufficient cause for the former would be an expansive power beneath the strata. Heat would provide such a power, and given that it is required for consolidation, there must necessarily be intense heat under the strata. Hutton was aware, moreover, that pressure increases the heat of gases. He argued, therefore, that as matter accumulates on the ocean floor, its weight must intensify the heat, causing the region to expand, thereby elevating the strata. He stressed that at this point we need not know the *cause* of the heat. Knowing simply that it exists in the required location and at the required intensity is sufficient. The alternative hypothesis of a retreating ocean could be ruled out on numerous grounds. Sediments, for example, must originally have been deposited in horizontal layers, but strata are found inclined at various angles. Had they not been raised but merely exposed as the ocean receded, they would surely have remained horizontal (Hutton, 1788, pp. 261–6; 1795a, pp. 120–30).

Hutton believed that the cause of this subterranean heat could be inferred from various observable geological phenomena. Mineral and metallic<sup>3</sup> veins in the strata and liquids expelled by volcanoes, for example, indicate that beneath the surface is a region of molten minerals and metals. Veins, he argued, are found in strata across the globe. And although volcanoes are presently extant only in particular areas, the number of mountains thought to be extinct volcanoes is clear evidence that they have formed, and therefore may form again, in any location. This, he asserted, attests to the universality of this subterranean region of molten matter, and thus to the generality of the cause of consolidation and uplift (Hutton, 1788, pp. 266–85; 1795a, pp. 130–64).

This, then, was the process by which solid land above sea had formed, and which the earth's matter will undergo repeatedly in order for it to maintain its purpose as a living world. In order of their formulation, then, we may divide Hutton's theory into the following eight constituents. (1) The earth's final cause is to provide a habitable world. (2) It was divinely contrived for this purpose. (3) Soil consists of eroded matter and is ultimately washed into the sea and deposited on the ocean floor. (4) The earth is analogous to an organic body in that it possesses a reproductive mechanism whereby eroded matter is repaired. (5) Strata are formed on the ocean floor out of this matter. (6) Sediments are fused and

consolidated by heat from subterranean molten materials and pressure from superincumbent sediment. (7) Strata are elevated and inclined by the expansion of these molten materials. (8) The process of erosion, deposition, consolidation, and uplift is cyclical.<sup>4</sup>

## 2. Predictions, confirmations, and accommodations

Hutton used the foregoing theory to make two important predictions. The first concerned veins of granite in strata. He probably already knew of granitic veins from the work of Swiss geologist Horace-Bénédict de Saussure, who had observed them pervading sections of strata in France in 1776 (de Saussure, 1779, pp. 531–6; Hutton, 1795a, pp. 318–9; 1899, pp. 90–142). Saussure subscribed to Abraham Gottlob Werner's "Neptunist" hypothesis, according to which the earth had once been covered by a great ocean which had slowly receded, exposing dry land (see Bowler, 2000, pp. 126–31). He believed that granites had originally formed via the dissolution and crystallisation of minerals in the ancient ocean. He ascribed the veins to a process of infiltration. Matter from nearby granite masses, he argued, was dissolved by rain water. The water flowed into fissures already extant in the strata. The matter then recrystallised in the fissures.

Contra Saussure, and indeed just about every other geologist at the time, Hutton believed that granite was of igneous origin. The fissures, moreover, did not exist prior to the veins but were coeval with them. Granitic veins, like other veins found in strata, were caused by molten materials breaking the strata and flowing into the fissures.

If this were correct, Hutton surmised, then at the junction between granite mountains and mountains composed of stratified rock, we should find veins extending continuously from the granite masses and intruding into the strata. Additionally, we should find fragments of strata suspended in the granite, but no fragments of granite in the strata. This latter point was important. Firstly, it would constitute compelling evidence that the granite had broken the strata when in a molten state. Secondly, it would prove that the granite masses had formed posterior to the strata. Neptunists held that granites were the first rocks to have formed in the ancient ocean. If this were the case, Hutton contended, then while we might find fragments of granite in the strata, we should find no fragments of strata in the granite. Finding the exact opposite would prove that the granite is younger (Hutton, 1794, pp. 77–9; 1795a, pp. 318–9; 1899, pp. 12–3; Playfair, 1805, p. 67).

Hutton's prediction was first confirmed in 1785 at Glen Tilt, a valley in the Scottish Highlands. Here, at the junction between the "schistus"<sup>5</sup> mountain to the south and the granite mountain to the north, he observed veins of granite extending continuously from the granite mass and intruding into the schistus. As predicted, numerous fragments of schistus were suspended in the granite, but no fragments of granite could be seen in the schistus (Hutton, 1794, pp. 79–80; 1899, pp. 1–13; Playfair, 1805, pp. 68–9) (Fig. 1). He found further confirmations at junctions between granite and schistus in Southwest Scotland and on the Isle of Arran in 1786–7 (Hutton, 1794, p. 80; 1899, pp. 31–62, 191–227; Playfair, 1805, p. 69). And in 1788, his would-be populariser Sir James Hall observed numerous exposures of the junction between a large area of granite and surrounding schistus at another location in Southwest

<sup>2</sup> Hutton (1788, p. 250; 1795a, p. 94) inferred this from the fact that pressure increases the boiling point of water.

<sup>3</sup> Hutton erroneously believed that metalliferous veins were the result of igneous intrusion rather than hydrothermal fluids as is now thought to be the case.

<sup>4</sup> It might be objected that (1), (2), and (4) were not really *parts* of Hutton's theory but merely *things he believed*. This, however, is unacceptably Whiggish. As various authors have argued (e.g., Lyons, 2006, p. 543; Harker, 2013, pp. 85–6), when assessing a given theory in relation to realism, we should avoid imposing modern ideals on the reasoning processes of past scientists. This must surely apply equally to what we do and do not consider to be constituents of their theories. (1), (2), and (4) are not the kinds of propositions we would ordinarily think of as constituents of *present-day* scientific theories. To Hutton, however, they were *essential* constituents. Consider (1), for example. Hutton (see, e.g., 1788, pp. 209–17, 285–304; 1795a, pp. 3–20, 269–310) made clear in numerous places that this was a central part of his theory, and that an explanation in terms of final causes was a crucial part of *any theory of any natural phenomenon whatsoever*. Indeed, his chief criticism of other theories of the earth was that they failed to account for final causes (Hutton, 1795a, pp. 269–85). The planned-but-never-written fourth volume of his book, moreover, was to be dedicated *entirely* to explaining final causes (see Hutton, 1785, pp. 28–30). Not to regard these constituents as internal to Hutton's theory, therefore, would be to impose modern-day standards of what constitutes a scientific theory onto Hutton.

<sup>5</sup> Hutton and his contemporaries used the term "schistus" to refer not only to what are now classified as schists but other heavily indurated alpine rocks such as quartzites and greywackes (see Geikie's footnote in Hutton, 1899, p. 6). When discussing Hutton's theory and observations and those of his contemporaries, I shall use their terminology. Later, when discussing modern-day geology, I shall use modern terms.





Fig. 1. Veins of granite intruding into neighbouring strata at Glen Tilt.

Scotland, all of which confirmed Hutton's hypothesis (Hall, 1794, pp. 8–9). Another Huttonian, John Playfair (1802, pp. 307–20), later reported several additional confirmations in his *Illustrations of the Huttonian Theory of the Earth*.

Hutton's second prediction concerned what geologists now term "angular unconformities": older sections of strata overlain by younger sections, the two sections resting at different angles, the former typically more inclined than the latter.<sup>6</sup> He had probably at this point read what he took to be descriptions of these in the work of Saussure, another Swiss Neptunist Jean-André Deluc, and two anonymous writers. These authors described horizontal or near-horizontal strata overlying sections of vertical or steeply inclined schistus. Saussure and Deluc both held that the secondary strata had formed from sedimentation upon primary rocks of an entirely different origin, the latter's structure appearing very unlike that of stratified rock. Saussure contended that they had formed from crystallisation as in the case of granite.<sup>7</sup> Deluc maintained that their origin was inexplicable, but discounted the possibility of sedimentation, confidently announcing that they "must be the effect of any other cause than simple successive depositions". Both believed they had not been raised or displaced but had formed in their present position and were exposed as the ocean receded (Deluc, 1779, pp. 607–32; 1780, pp. 195–224—quotation from p. 210 [my translation]; de Saussure, 1779, pp. 179–90; Hutton, 1795a, pp. 371–420).

Hutton's theory supplied him with what was at the time a rather unorthodox, but in his view far more plausible, explanation. The difference between the two sections, he urged, was simply that the primary strata had undergone the process of consolidation and uplift *more than once*. That is, the primary strata had first formed from sedimentation. They were consolidated by heat and pressure and uplifted by the expansion of molten matter. This displaced them from their original, horizontal position, causing them to take on a vertical or steeply inclined posture. Following this, they had subsided such that they were again below sea level but maintained their inclination.<sup>8</sup> The secondary strata then formed from sediments deposited horizontally on top of the vertical or steeply

inclined primary strata. They, too, were consolidated by heat and pressure. Finally, both sections were uplifted together by the expansion of molten matter (Hutton, 1795a pp. 371–6).

Hutton sought to test his hypothesis with observations of his own. He reasoned that at junctions between alpine schistus and low-country sedimentary rock, we should find horizontal or slightly inclined sections of the latter superincumbent upon vertical or steeply inclined sections of the former. Additionally, since the cause of elevation and consolidation are the same, we should find that the primary strata, which are more inclined, are also more consolidated, for they have been more affected by that cause. For the same reason, we should find also that the primary strata are more deformed and include more and larger igneous veins than the secondary strata (Hutton, 1795a, pp. 419–28). Hutton stressed that it was important these latter characteristics be observed, for they were clear signs that the primary strata have been uplifted more than once. "[T]his conclusion" (that strata have been deposited on strata which have previously been consolidated and elevated), he asserted,

is not of necessary consequence, without examining concomitant appearances, and finding particular marks by which this operation might be traced; for the simply finding horizontal strata, placed upon vertical or much inclined schistus, is not sufficient, of itself, to constitute that fact (Hutton, 1795a, pp. 419–20).

In 1787, following an inconclusive example near Lochranza on the Isle of Arran (Hutton, 1795a, pp. 429–430; 1899, pp. 234–7; Playfair, 1805, pp. 69–71), Hutton's prediction was confirmed when, on the banks of Jed Water near Jedburgh where the schistus of the River Tweed meets the sandstone of the Teviot, he discovered vertical schistus overlain by horizontal sandstone (Fig. 2). He observed another, similar unconformity shortly afterwards in a brook to the south of the river (Hutton, 1795a, pp. 430–2, 443–4, plate 3; Playfair, 1805, p. 71). As predicted, the primary strata in both instances were more consolidated, distorted, and veined than the secondary strata. Hutton's theory enabled him to explain various other appearances, too. The ends of the vertical strata, for example, were cut off smoothly, with little variation in height. This evidently resulted from the primary strata having been exposed long enough for the edges, which must have been sharp and uneven when the strata were broken and displaced, to have been greatly eroded before the secondary strata were deposited. Relatedly, between the two sections was a layer of conglomerate composed of the same substances as the primary strata. This, he

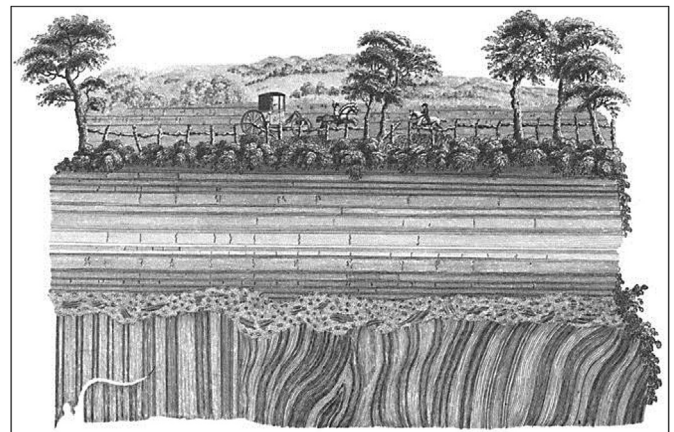


Fig. 2. John Clerk of Eldin's engraving of Hutton's unconformity at Jedburgh.

<sup>6</sup> There are various kinds of unconformity. Since this paper is concerned solely with *angular* unconformities, I shall hereafter refer to them simply as "unconformities".

<sup>7</sup> Saussure later abandoned this view, arguing that they formed from sedimentation and were subsequently folded as a result of lateral pressure. This, however, was not until long after the publication of Hutton's theory (Rudwick, 2005, pp. 109–10).

<sup>8</sup> Hutton did not specify how strata might subside, noting only that whatever supports them must sometimes fail or give way (Hutton, 1785, pp. 373–5).

argued, had obviously formed from parts of the primary strata which had broken off during elevation and eroded prior to the sandstone being deposited (Hutton, 1795a, pp. 433–44).

The following year, Hutton travelled to Southeast Scotland in search of the junction between the schistus of the Lammermuir Hills and the low-country sandstone to the north. He found his first exposure at Pease Burn where he observed slightly inclined sandstone overlying near-vertical schistus (Hutton, 1795a, pp. 453–6). Following this, accompanied by Playfair and Hall, he boarded a boat at Dunglass Burn and sailed south along the coast, “certain of meeting with the junction” between the schistus and sandstone where the rocks had been exposed by the sea. They found their first part of the junction at St Helen’s, where they observed an unconformity similar to that seen at Pease Burn. Then, slightly further south at Siccar Point, they discovered what would become Hutton’s most famous unconformity. Here, near-vertical schistus is overlain by slightly inclined sandstone, the latter having been partly eroded exposing the ends of the schistus (Fig. 3). After this, they saw only steeply inclined schistus till Redheugh, where they observed another superincumbent section of near-horizontal sandstone (Hutton, 1795a, pp. 455–9—quotation from p. 456). Again, in the foregoing instances, the “particular marks” that Hutton predicted were present. Once again, too, he was able to account for certain other appearances. These latter unconformities differed from those at Jedburgh in that the primary strata had sharper, uneven edges, the longer strata extending into the bottom layers of sandstone. Additionally, the conglomerate between the two sections was composed of larger fragments of schistus than that seen at Jedburgh. The fragments were also sharper and less rounded. These differences, Hutton explained, were simply the result of the schistus having been exposed for a shorter period. The ends of the strata and the fragments had begun to erode, but had evidently done so for a shorter time before the sandstone was deposited (Hutton, 1795a, pp. 456–71).

Playfair (1802, pp. 212–9) would later recount a further six confirmations from his own searches for junctions between alpine and low-country strata: one at Torbay in Southwest England; two on the coast of Banffshire in Northeast Scotland; one on Dunmallet in the northern Lake District; one at the foot of Ingleborough near Ingletton in the Yorkshire Dales; and one nearby at Thornton Force on the River Greta (Fig. 4).

That Hutton made *predictions* about these phenomena may surprise some readers. This is because the mischaracterisation of him as a rigorous empiricist who constructed his theory on a



Fig. 4. Playfair's unconformity at Thornton Force.

foundation of painstakingly collected field observations, though thoroughly discredited, is still well entrenched. As Gould (1987, pp. 66–73) observes, this standard misreading of Hutton began in the early-nineteenth century with his popularisers stripping his theory of its various theological and metaphysical underpinnings. It was given substance thirty years later by Charles Lyell (1830, 1–91), whose self-serving rewrite of the history of geology juxtaposed an empirically-grounded Huttonian theory against the purportedly speculative and theologically-driven ideas of his contemporaries and predecessors. It became canonised at the end of the century in Archibald Geikie's (1897, pp. 150–200) *Founders of Geology* in which Hutton emerged as the heroic “modern” in a similarly “heroes and villains” approach to history (see Oldroyd, 1980, pp. 444–7). And it is perpetuated to this day in the pages of numerous geology textbooks (for examples see Gould, 1987, pp. 68–70; Montgomery, 2003, pp. 500–1). According to the standard myth, Hutton’s observations of granitic veins and unconformities were key pieces of empirical data on which he constructed his theory. Veins intruding from granite masses into neighbouring strata were conclusive evidence of widespread violent uplift resulting from the expansion of molten materials. Unconformities were proof of successive cycles of erosion, deposition, consolidation, and uplift. He induced these two central parts of his theory from the two crucial observations.

As a number of historians have urged (e.g., Gould, 1987, pp. 60–79; Laudan, 1987, pp. 128–9; Bowler, 2000, pp. 125–38; Oldroyd, 1996, pp. 92–6; Rudwick, 2005, pp. 164–8; 2014, pp. 68–73), and as the above reconstruction of Hutton’s reasoning indicates, this picture is upside down. Although he did appeal to various empirical data, this amounted to nothing more than was widely known among intellectuals at the time rather than the result of sustained field inquiry. Instead, the theory was derived largely *a priori* from first principles about final causes. As Rudwick (2005, p. 164 [emphasis original]) assesses, Hutton “proposed a highly abstract model of how the earth *must* work, if ... it was ‘a thing formed by design’ ..., mak[ing] his system public *before* undertaking any fieldwork specifically directed towards finding empirical support for it”. The two crucial pieces of evidence were indeed his observations of granitic veins and unconformities. These, however, were not data from which he *induced* his theory, but confirmations of *predictions* made on the basis of it. As Rudwick (2005, p. 166 [emphasis original]) avers, when Hutton “searched ... for decisive features that his system led him to expect ..., he was trying to verify predictions *deduced* from his hypothetical model”. And as Rachel Laudan (1987, p. 129 [emphasis added]) makes clear, “[Hutton’s] fieldwork was directed to the *testing* of his hypotheses, not to their generation”.



Fig. 3. Hutton's unconformity at Siccar Point.



As Gould (1987, p. 70) emphasises, “[s]imple chronology” is enough to show that this is the case. Hutton presented the theory to the Royal Society of Edinburgh on 7th March and 4th April 1785. Immediately afterwards, he circulated an abstract describing the theory essentially in its final form (Hutton, 1785). This was certainly prior to his visit to Glen Tilt. For in his account of the expedition, entitled “Observations made in a Journey to the North Alpine part of Scotland in the Year 1785” (Hutton, 1899, pp. 1–30), he stated that he visited the area “this Harvest”, indicating that it was late September (Hutton, 1899, p. 11). Additionally, when reporting his observations of granitic veins to the Society in 1790, he clearly stated that the observations were made “[s]ince reading the paper upon the theory of the earth” (Hutton, 1794, p. 77). He had previously only seen granite in the field at two uninformative locations. He had never seen either veined granite or any junction between granite and stratified rock. As he later recounted, “I just saw it, and no more, at Peterhead and Aberdeen; but that was all the granite I had ever seen when I wrote my Theory of the Earth. I have, since that time, seen it in different places; because I went on purpose to examine it” (Hutton, 1795a, p. 214).

As for unconformities, according to Playfair (1805, p. 69), Hutton did not observe his first one at Lochranza until “summer 1787”. Furthermore, at the beginning of the chapter in which he considered Deluc’s and Saussure’s descriptions of the phenomenon, he made it clear that he intended to use his – already complete – theory to explain it, and then to confirm his explanation – and by extension his theory – with observations in the field. “The present object of our contemplation”, he wrote,

is the alternation of land and water upon the surface of this globe. It is only in knowing this succession of things, that natural appearances can be explained; and it is only from the examination of those appearances, that any certain knowledge of this operation is to be obtained (Hutton, 1795a, p. 372).

And at the beginning of the subsequent chapter where he recounted his observations of unconformities he promised “to treat of this subject from observations of my own, which I made *since forming that opinion* [i.e., that secondary strata have been deposited on top of previously consolidated and uplifted primary strata]” (Hutton, 1795a, p. 421 [emphasis added]).

Hutton’s theory, moreover, was evidently complete long before he presented it to the Society. Thus, he probably made his crucial observations not just months or years but *decades* after formulating his theory. Playfair (1805, p. 59), for instance, suggested that the theory was complete not long after 1760. Joseph Black, too, stated in 1787 that “Dr Hutton had formed this system ... more than twenty years ago” (quoted in Dean, 1992, p. 49). Granitic veins and unconformities, then, were not data from which Hutton derived his theory, but phenomena which he believed his – already complete – theory could explain, and which should be observed if the theory were correct.

### 3. The case’s relevance to the realism debate

Hutton’s predictions were of precisely the kind that is relevant to the realism debate. They were, of course, successful. They were also novel. Typically, in the realism literature, a prediction is considered novel if the phenomena predicted were not employed in the formulation of the theory used to make the prediction (see, e.g., Lyons, 2002, p. 69; Psillos, 1999, pp. 100–2). Playfair and Black dated the completion of Hutton’s theory at some time in the 1760s. He most likely first learned of granitic veins and unconformities through Saussure and Deluc, whose observations were not published until 1779–80. The first full version of Hutton’s theory,

moreover, published initially as an author’s separate early in 1786 (Dean, 1992, p. 25) and then in the *Transactions of the Royal Society of Edinburgh* in 1788 though probably written much earlier,<sup>9</sup> mentioned neither phenomenon. Hutton discussed veins more generally as evidence for the cause of consolidation and uplift (Hutton, 1788, pp. 259–61, 266–71). He also noted that fragments of strata in some veins indicated that molten matter exerts a force on the strata (Hutton, 1788, pp. 270–1). But he did not discuss veins of granite. Where he did discuss granite, he considered only a small sample, referring to its structure as evidence that it had previously been in a fluid state (Hutton, 1788, pp. 254–7). As to unconformities, although he appealed to strata being inclined at various angles as evidence of their having been elevated and displaced (Hutton, 1788, pp. 263–6), he nowhere discussed secondary strata resting unconformably on primary strata. He also mentioned neither Saussure nor Deluc, suggesting that he had not at this point read their descriptions of these phenomena.

When discussing novel predictions, philosophers typically distinguish between two different kinds of novelty: *temporal*-novelty; and *use*-novelty (see, e.g., Earman, 1992, pp. 113–17; Psillos, 1999, pp. 100–2; Lyons, 2002, p. 69; 2006, p. 544). A prediction is *temporally*-novel if the phenomenon predicted was not known at the time the prediction was made. It is *use*-novel if the predicted phenomenon was known about at the time of the prediction but no information about the phenomenon was employed in the formulation of the theory used to make the prediction. It is not entirely clear which category Hutton’s predictions fit into. Although he did not state explicitly whether he made the predictions before or after reading Saussure’s and Deluc’s observations, he did discuss these authors’ observations before recounting his own, indicating that he probably made the predictions after having read them. Saussure’s and Deluc’s writings, then, probably influenced the predictions. These authors, however, described and interpreted the phenomena very differently to Hutton. Their descriptions, moreover, lacked various important details that Hutton predicted, such as the specific locations of the phenomena, the fragments of strata in granitic veins and corresponding lack of fragments of granite in the strata, and the specific differences between the primary and secondary strata of unconformities. It should be stressed, however, that whether the predictions were temporally- or use-novel is relatively unimportant here. What ultimately matters is that the phenomena were in no way used in the formulation of Hutton’s theory. This is sufficient for the predictions being instances of novel success.

Hutton’s accounts of the ends of the primary strata and layers of conglomerate between the two sections were arguably novel too in that they resemble what Psillos (1999, p. 101) refers to as “novel accommodation”. Novel accommodation, Psillos argues, is “any case in which a known fact is accommodated within the scope of a scientific theory, but no information about it is used in its construction”. He contrasts this with “ad hoc accommodation”. An accommodation is ad hoc if either (a) information about the phenomenon was used in the formulation of the theory, or (b) the theory was modified solely to account for the phenomenon. Hutton’s explanations of these phenomena satisfy neither of Psillos’s “ad hocness conditions”. The phenomena were not employed in the theory’s formulation. And Hutton accommodated them without modifying his theory. The particular combinations of uneven ends of primary strata and a coarse-grained conglomerate on the one hand, and even ends of primary strata and a finer-grained conglomerate on the other, were precisely what should be

<sup>9</sup> Playfair indicated that there were several much earlier drafts (see Dean, 1992, p. 6).

expected if the primary strata had been raised and broken and then subsequently eroded in varying degrees. Nothing needed to be added to Hutton's system of erosion, deposition, consolidation, and uplift to account for these phenomena. They were accommodated, in Psillos's words, "within the scope" of the theory. These explanations, then, insofar as novelty is considered important by the realist, arguably warrant some further degree of realist commitment to whichever constituents of the theory were responsible for them.

Another important factor which, as Vickers (2013, p. 195) notes, is often underappreciated in the literature, is that for a case to be relevant to the realism debate, a theory's prediction(s) must not only be successful and novel but also sufficiently *impressive* to warrant realist commitment, or some degree thereof, to the responsible constituent(s) of the theory. If a prediction is vague enough that it has a high probability of being successful by mere chance, then it does not warrant such commitment. Vickers (2013, pp. 195–8) nicely illustrates this point with the case of Immanuel Velikovsky's prediction that Venus will be found to be "hot". Though successful and novel, Vickers argues, Velikovsky's prediction does not warrant realist commitment to the responsible constituents of his theory because it was vague enough to have had a reasonably high probability of being successful regardless of whether the relevant constituents are true. So although the responsible constituents of Velikovsky's theory are, by today's lights, wildly false, it does not constitute a counterexample to selective realism.

Hutton's predictions were far more specific and impressive. Veins of granite should be observed in specific locations. Fragments of strata should be suspended in the veins. No fragments of granite should be found in the strata. According to the prevailing Neptunist theory, granites were the very first rocks to have formed, and were the main foundation on which all other rocks had formed. They were also not thought to be of igneous origin. Indeed, igneous activity was generally believed to be a relatively recent and highly localised (i.e., confined to areas with active volcanoes) phenomenon (Young, 2003, pp. 62–4). On such a view, finding veins extending from granite masses into neighbouring strata, and especially finding fragments of strata suspended in the granite but no fragments of granite in the strata, was extremely unlikely. As Young (2003, p. 62) states in his study of the history of igneous petrology, eighteenth-century geologists "did not expect to see evidence for what we now regard as widespread subsurface intrusive activity". Hutton's prediction, then, especially the part about there being fragments of strata suspended in the veins but no fragments of granite in the strata, ran directly counter to what was widely believed about granite at the time. It was therefore especially risky, and thus precisely the kind of prediction that realists would want to regard as unlikely to be successful if the responsible constituent(s) of the theory are not at least approximately true.<sup>10</sup>

The prediction about unconformities also had several very specific details. Unconformities should likewise be found in particular locations. There should be significant angular discordance between the primary and secondary strata. The primary strata should be more consolidated, distorted, and veined than the secondary strata. As with his interpretation of granite, it is important to remember that at this time Hutton's idea of successive cycles of uplift was thoroughly unorthodox. As Rachel Laudan (1987, p. 121) puts it, "Hutton's theory flew in the face of all the conventional wisdom about geology that had been slowly built up in the

eighteenth century". Widespread uplift of the strata was not generally believed to occur. Uplift was thought to result only from volcanic activity, which, as I have noted above, was believed to be recent and highly localised. Unconformities, and inclined strata more generally, were therefore especially puzzling for Neptunists. Indeed, Deluc (1780, pp. 206–16), having previously held that all strata had formed through sedimentation, came to abandon this view and posit a non-sedimentary origin for the primary strata as a direct result of observing these phenomena. In contrast, for Hutton, unconformities were implied by his theory of successive cycles of erosion, deposition, consolidation, and uplift. They were something that should be observed if his theory were correct. His particular, unorthodox interpretation of them, moreover, allowed him to predict (a) the localities at which they should be found, and (b) the specific differences that should be observed between the primary and secondary strata.<sup>11</sup> It also enabled him to explain a variety of other associated phenomena.

As Peters (2014, pp. 389–90) observes, it is intuitive to think that empirical successes are impressive in such a way as to warrant realist commitment to the responsible constituents of the relevant theory insofar as they predict and/or explain a variety of phenomena significantly larger than the empirical basis of the theory. In both these cases, Hutton was able to do precisely this. He was able, that is, to use a theory with a relatively small empirical base to predict and explain a variety of very specific phenomena. To paraphrase Peters (2014, p. 389), Hutton "got more out than he put in".

When assessing the impressiveness of empirical successes in relation to scientific realism, it is important also to consider the attitudes of scientists involved in and influenced by these successes. In his discussion of Arnold Sommerfeld's prediction of the fine-structure of hydrogen spectral lines, for example, Vickers (2012, pp. 6–8) points to Albert Einstein's and Paul Epstein's conversion to the old quantum theory in light of Sommerfeld's prediction as evidence for the case's significance for the realism debate. One reason why such considerations are important is that they help protect against historical bias. That is, given our knowledge that a theory has been superseded, we might be apt to misjudge whether the theory's empirical successes were sufficiently impressive to warrant realist commitment to the responsible constituent(s) of the theory. Considering whether scientists working at the time saw the successes as compelling evidence for the theory enables us to judge more reliably whether, given what was known at the time, the successes warranted realist commitment to the responsible constituent(s) in a way that is analogous to present-day realists' inference from empirical success to the (approximate) truth of responsible constituents of current theories.

The first thing to note about Hutton in this regard is that he saw his predictions as stringent tests of his theory. As Playfair (1805, p. 67) recounted regarding the prediction about granite, "Dr Hutton was anxious that an *instantia crucis* might subject his theory to the severest test". Such feelings were not unwarranted. Had there not been veins of granite intruding into strata at junctions between the two, or had they not been contiguous with the masses, or not contained fragments of strata, or had there been fragments of granite in the strata, then the idea that molten granite had broken the strata, and by extension the idea of widespread uplift resulting from igneous intrusion, would have looked implausible. Likewise, had alpine strata not been overlain unconformably by low-country strata at junctions between the two, or had the primary strata not

<sup>10</sup> In Bayesian terms, given the unorthodoxy of Hutton's view of granite,  $P(E|T)$  was very low. Therefore, after the phenomenon was observed,  $P(T|E)$  was significantly higher than the prior probability of the theory.

<sup>11</sup> Again, for those inclined towards Bayesian reconstruction, given the unorthodoxy of Hutton's interpretation of unconformities,  $P(E|T)$  was very low. Thus, after the observations,  $P(T|E)$  was significantly higher than the theory's prior probability.



been more consolidated, distorted, and veined than the secondary strata, the theory of successive cycles of uplift resulting from igneous activity would have been undermined. Although the phenomena were not strictly *entailed* by the theory, they were very likely consequences of it. Had the predictions been unsuccessful, although the theory would not necessarily have been falsified, it would surely have been rendered significantly less plausible.

A closely-related and equally important point is that Hutton's successes were seen as compelling evidence for his theory. As Oldroyd (1996, p. 95) asserts with reference to unconformities, "Hutton made a successful prediction, and such success generally tends to attract assent to one's ideas. It did so in this case among Hutton's friends in Edinburgh, even though his theory had many speculative and uncertain features". Indeed, describing his and Hall's reaction to observing the unconformity at Siccar Point, Playfair (1805, p. 72) recounted:

On us who saw these phenomena for the first time, the impression made will not be easily forgotten. The palpable evidence presented to us, of one of the most extraordinary and important facts in the natural history of the earth, gave a reality and substance to those theoretical speculations, which, however probable, had never till now been directly authenticated by the testimony of the senses. We often said to ourselves, What clearer evidence could we have had of the different formation of the rocks, and of the long interval which separated their formation, had we actually seen them emerging from the bosom of the deep?

Perhaps most strikingly, Hall, having previously rejected Hutton's theory entirely, became converted as a direct result of these successes, subsequently becoming one of the theory's most influential proponents (see Hall, 1805; 1812; 1815; Ranalli, 2001, pp. 66–71). In a paper presented at the Royal Society of Edinburgh in 1805 he related that he

was induced ... by the numerous original facts which his system had led him to observe, to listen to his arguments, in favour of opinions which I then looked upon as visionary ... After three years of almost daily warfare with Dr Hutton, on the subject of his theory, I began to view his fundamental principles with less and less repugnance. There is a period, I believe, in all scientific investigations, when the conjectures of genius cease to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, against its improbability as an hypothesis (Hall, 1812, p. 75).

A final point which I have already touched upon but which is worth stressing further is that Hutton was able to account for these phenomena in a way far more impressive than the competing Neptunist theory. Harker (2013) has suggested that we conceptualise success in relation to realism in comparative terms. That is, a theory should be considered successful in a way that warrants a degree of realist commitment to the responsible constituents just in case it can account for phenomena in a way that rival theories cannot. Granitic veins were problematic for Neptunists. Saussure's theory of infiltration was precisely the kind of hypothesis that would satisfy Psillos's second ad hocness condition, since it was introduced solely to accommodate that particular phenomenon. Additionally, as Playfair (1802, pp. 88–9, 314–5, 338–9) pointed out at the time, it was replete with problems. Granite did not appear to be water-soluble. Granitic veins often point *upwards* into strata, which is surely impossible if veins are only filled from above. And if both granite and schistus are formed via crystallisation, then

the Neptunist must explain why the same process of infiltration does not also produce veins of the latter.

Unconformities were equally problematic. As we have seen, Neptunists like Saussure and Deluc accounted for the verticality or steep incline of the primary strata by positing a non-sedimentary origin. In many cases, though, as is evident in the examples Hutton and Playfair observed, the secondary strata, which even Neptunists believed to be sedimentary, were also inclined. To avoid admitting the possibility of widespread uplift, Neptunists had to introduce various hypotheses to account for non-horizontal sedimentary strata. Some, for example, appealed to a "transition" phase in which deposited sediments mixed with crystallising matter, causing the strata to take on a posture intermediate between the vertical primary rocks and horizontal sedimentary strata (see, e.g., Jameson, 1808, pp. 55–7, 145–52). Again, this was an ad hoc hypothesis, introduced specifically to accommodate this particular phenomenon. Contrastingly for Hutton, non-horizontal secondary strata were unsurprising given that the strata had been subject to an additional period of uplift. Considering success in comparative terms, then, at least with regard to these phenomena, Hutton's theory was successful in a way that, on Harker's view, should warrant a significant degree of realist commitment to the responsible constituents of the theory.

#### 4. The true, the false, the working, and the idle

I shall now consider which constituents of Hutton's theory are and are not approximately true and which were responsible for its success. When assessing the constituents, I will endeavour to be as charitable as possible, even where this involves accepting as approximately true constituents which may upon further analysis turn out not to be approximately true. If any radically false constituents were responsible for Hutton's success, then the case will, at least *prima facie*, constitute a counterexample to selective realism, and will at the very least warrant further attention.

First, then, let us consider the eight constituents listed above (p. 7). Of these, (1), (2), and (4) are uncontroversially radically false. We no longer believe that the earth has a final cause. Indeed, final causes arguably have no place whatsoever in modern science. Likewise, the belief that the earth was divinely contrived, while still held in religious circles, is not part of our modern scientific worldview. And the earth is not generally thought to be analogous in any significant way to an organic body.

(3), (5), and (8), on the other hand, are uncontroversially at least approximately true. Although we now have a more detailed picture of soil formation, soil scientists today ultimately agree that it is composed mainly of eroded matter, and that most of it is eventually washed into the ocean or at least into other bodies of water (see, e.g., Kutílek & Nielsen, 2015, pp. 31–70). The vast majority of sedimentary strata are indeed thought to have formed mainly from eroded matter, and while sedimentation does not always occur in marine environments, most terrestrial depositional environments are nevertheless aqueous. So although not all strata are formed in the ocean, it is true that they typically form in bodies of water. Finally, though the particular cyclical process Hutton had in mind is significantly different from that of our current geological theories, there is an important sense in which Hutton was right about the cyclicity of geological processes. Strata forming on the ocean floor will become new continents as the oceans close. They will then be eroded and deposited again, forming new strata. Indeed, geologists often talk of "tectonic cycles", the "rock cycle", etc.

(6), and (7) are questionable. Regarding (6), only *metamorphic* rock has undergone anything like the combination of intense heat and pressure to which Hutton ascribed consolidation. Additionally, metamorphism is very different from the process Hutton had in

mind. He held that heat brings sediments to a fluid state from which they are fused and consolidated. Yet, firstly, metamorphism is undergone not by loose sediments but by sedimentary strata (and also igneous rocks) which have long since solidified. Secondly, it is a *solid-state* transformation which occurs at temperatures well below liquidus. The heat at depths where metamorphism occurs is nowhere near intense enough to induce fluidity. Rather, it causes strata to recrystallise by releasing volatiles in minerals which are stable at lower temperatures (Frost & Frost, 2014, pp. 158–9). So not only are strata already consolidated prior to undergoing metamorphism, but the process itself is markedly different from Hutton's view of consolidation. Nevertheless, metamorphism does result from heat and pressure and does cause strata to become denser. And although metamorphism does not induce fluidity, it does induce a certain amount of plasticity, causing strata to become deformed. So insofar as it was used to predict and/or explain these rocks' greater density and deformation, (6) might be regarded as approximately true with respect to metamorphic rock.

Regarding sedimentary strata, however, (6) is surely radically false. Lithification – consolidation of sediment – is typically brought about by a combination of *compaction* and *cementation*. Compaction results from pressure exerted by superincumbent sediment. The pressure causes sediments to rearrange themselves and causes some sediments to become deformed. This packs the sediments more tightly together and decreases the amount of pore space between them, driving water out of the pores. At greater depths, sediments can begin to dissolve at their points of contact and become sutured together – this is called “pressure solution”. Cementation occurs when water flows through the pore space, precipitating minerals – “cements” – into it. Cements fill the pore space and bind the sediments together into a solid framework. The most common cements are silica and calcite. The ions that precipitate as cement are produced by the dissolution of minerals either within the sediment or elsewhere with the ions being carried into the sediment by water (Prothero & Schwab, 2013, pp. 121–4). These processes contrast sharply with Hutton's view of consolidation. Pressure is involved in compaction, but its primary role here is in rearranging sediments and driving out interstitial water rather than preventing the combustion or evaporation of sediment as Hutton believed. And while heat plays a role in cementation by affecting the solubility of minerals, this role is obviously significantly different from the role Hutton ascribed to heat. The most common sedimentary cements, moreover, are soluble at low temperatures.

(7) is also arguably not even approximately true. Under the earth's surface is indeed hot. But it is not composed of molten materials, at least not until the outer core – nearly 3000 km beneath the surface. Although the asthenosphere has a degree of plasticity, the mantle is essentially solid. Molten matter is only found near the surface at plate boundaries and so-called “hot spots”. Thus, although the kind of igneous intrusion that Hutton proposed does occur and does cause a certain amount of uplift and displacement of strata, it is far from being the main cause of such phenomena, the most significant cause being tectonic activity at convergent plate boundaries. Insofar as it was used to predict or explain any instance of uplift and/or displacement of strata that was not in fact the result of igneous intrusion, then, (7) must be considered radically false.

Turning now to the question of which constituents were responsible for Hutton's empirical success, with regard to the prediction about granitic veins, it seems the constituent most obviously and directly involved here was (7). That strata are elevated by molten materials exerting a force beneath them implied that molten granite should exert pressure on the strata, causing them to fracture while simultaneously flowing into the

fissures. Thus, at junctions between granite and strata, we should find veins extending from the granite masses into the strata. Since the molten granite has broken the strata, we should find fragments of strata suspended in the veins. But because the granite is younger than the strata, we should find no fragments of granite in the strata.

As to the prediction about unconformities, the constituents most obviously and directly involved seem to be (3), (5), (6), (7), and (8). The process of erosion, deposition, consolidation, and uplift described in (3), (5), (6), and (7), combined with its cyclicity (8), implied that sediments will be deposited horizontally upon strata which have previously been consolidated, elevated, and displaced from their original, horizontal position. The sediments will be consolidated. Then both sections will be elevated together. Thus, at junctions between low-country and alpine strata, we should find vertical or near-vertical sections of the latter overlain by horizontal or near-horizontal sections of the former. And because the primary strata have been subjected to the cause of consolidation and uplift more than once, we should find that as well as being more inclined, they are also more consolidated, distorted, and veined than the secondary strata.

As noted above, (3), (5), and (8) are candidates for approximate truth and are thus unproblematic for selective realism. (6) and (7), on the other hand, seem, *potentially* at least, to be problematic. As I have argued, (6) appears to have been involved in the prediction about unconformities. Hutton believed that both the primary and secondary strata were consolidated by heat and pressure bringing sediments to a fluid state and fusing them together, the primary strata having undergone the process twice. According to current views, though, only the primary strata have undergone anything like this process, and only after having been lithified. So while Hutton's theory of heat and pressure can perhaps account for the deformation and further consolidation of the primary strata, it cannot account for its original lithification, nor for the lithification of the secondary strata.

Here, the realist might emphasise the role of pressure in lithification. Despite the different role pressure plays in compaction to that which Hutton ascribed to it, the realist might argue, compaction does nevertheless result directly from pressure exerted by superincumbent sediment. This part of (6), the realist could suggest, is at least approximately true. This part was doing the work in deriving the prediction. Heat, and the specific role Hutton attributed to pressure, were idle.

Even notwithstanding the very different role of pressure, however, compaction alone is only sufficient to lithify fine-grained sediments such as shales and mudstones. The coarse-grained sandstones, greywackes, and conglomerates with which Hutton and Playfair were chiefly concerned require cementation. Even pressure solution is insufficient, since no amount of pressure can generate enough suturing to lithify these coarser sediments (Prothero & Schwab, 2013, p. 123). Hutton was aware that pressure alone is insufficient. However, he explicitly rejected the possibility of sediments being consolidated by precipitating minerals (Hutton, 1795a, pp. 273–4). He believed that sediments had somehow to be brought to a fluid state to be consolidated, and that the only possible cause of this was heat. Heat, then, cannot be considered idle. And since heat is not involved in any significant way in lithification, the role of (6) in Hutton's prediction appears problematic for selective realism.

Another possible response for the realist is to deny that (6) was involved in the prediction at all. (6), the realist could argue, pertains to the processes by which strata are formed. Unconformities concern the relationship between distinct sections of strata. The cause of consolidation, therefore, is irrelevant to the prediction, and so (6) was idle.

While (6) may not be directly relevant to the *relationship* (i.e. angular discordance) between sections of strata, it does seem to be relevant to the various differences Hutton predicted should be observed between the two sections, for these differences followed directly from the cause of consolidation and uplift being the same. So if (6) is considered idle, we may lose this part of the prediction. Moreover, in Hutton's reasoning, (6) was very closely connected with (7). Recall (pp. 6–7) that Hutton considered two possible causes of consolidation: (a) dissolution and crystallisation; and (b) heat and fusion. (a) could be ruled out, since strata contain substances that are water-insoluble. The cause of consolidation, therefore, was necessarily (b). It was from this conclusion that he derived (7). To produce land above sea, either the ocean has receded or the strata have been elevated. Ruling out the former on various grounds, he concluded the latter. A sufficient cause of elevation would be an expansive power beneath the strata. Heat would provide such a power, and since it is the only possible cause of consolidation, there must necessarily be heat under the strata. Adhering to the Newtonian principle of admitting no more causes than are necessary to explain a given phenomenon, he concluded that heat must also be the cause of elevation. (He inquired into the *cause* of this heat only *after* having reached this conclusion.) So although (6) was arguably *less directly* involved in the prediction about angular discordance than other constituents, it is not clear that it can be considered idle, because Hutton very likely would not have arrived at (7) without it.

Turning now to (7) itself, this constituent seems unproblematic with respect to the prediction about granitic veins, for the veins in the strata and fragments of strata in the veins were indeed the result of igneous intrusion. For the prediction about unconformities, though, it does seem problematic. It is now believed that unconformities are caused by various different processes, many examples being the result of more than one process (Miall, 2016). The unconformities observed by Hutton and Playfair are now thought to have resulted from orogenic tectonism, processes of mountain building brought about by the interaction of plates. Most of these unconformities formed during the Caledonian Orogeny, a series of tectonic events associated with the closure of the Iapetus Ocean between Laurentia, Baltica, and Avalonia during the Cambrian, Ordovician, Silurian, and Devonian periods (see McKerrow, Mac Niocaill, & Dewey, 2000).

The primary schists of Playfair's unconformities in Banffshire and Hutton's unconformity near Lochranza are composed of Dalradian sediments originally deposited on the eastern margin of Laurentia during the Late-Precambrian and Early-Cambrian. During the Grampian phase of the orogeny in the Late-Cambrian–Early-Ordovician, an intra-oceanic south-directed subduction zone caused an island-arc complex to form to the south of Laurentia. Further closure of the Iapetus brought the complex into collision with Laurentia, blocking the subduction zone and causing ophiolites (sections of oceanic crust and underlying mantle) to be obducted onto the continental margin. This caused the Dalradian strata to be buried, folded, and metamorphosed. Further convergence of the complex with Laurentia brought about folding of the ophiolites and underthrusting of the strata, bringing it to a more vertical posture. The strata were then exhumed by erosion, with those in Banffshire being overlain by Old Red Sandstone during the Mid-Devonian, and those at Lochranza being overlain by conglomerate, sandstone, and mudstone during the Early-Carboniferous (Stephenson and Gould, 1995, section 3; Jones & Blake, 2003, pp. 47–72; Young & Caldwell, 2009; Chew & Strachan, 2014, pp. 73–5).

Hutton's most famous unconformities at Jedburgh and Siccar Point, along with the others observed in the Southern Uplands terrane, formed during a later – unnamed – phase of the orogeny.

The primary greywackes formed on the ocean floor to the south of Laurentia during the Ordovician and Silurian periods. As the above island-arc complex converged with Laurentia, a second, north-directed subduction zone formed on the oceanward side of the complex. As the ocean closed, the northward subduction of the oceanic plate caused the strata to form an accretionary prism at the margin of the overriding continent. Essentially, sections of strata were scraped off the ocean floor and stacked onto the continental plate in the ocean trench. As the trench filled, lateral force created by the subducting oceanic plate caused the strata to be rotated anticlockwise into an almost vertical posture. The rocks buried at greater depths in the prism were metamorphosed, exhumed by erosion, and later overlain with Old Red Sandstone during the Upper-Devonian (Leggett, McKerrow, & Eales, 1979; Jones & Blake, 2003, pp. 80–5; Stone & Merriman, 2004; Stone, McMillan, Floyd, Barnes, & Phillips, 2012a,b; Stone, 2012; 2014; Miall, 2016, pp. 28–9).

Playfair's unconformities at Dunmallet, Ingleton, and Torbay formed from similar tectonic processes. The primary slates and greywackes at Dun-Mallet, Ingleborough, and Thornton Force were originally formed in the Iapetus at the northern margin of Avalonia during the Early-Ordovician. They were folded and metamorphosed during the Acadian Orogeny, a mountain building process resulting from the oblique convergence of Avalonia and Laurentia during the Early-to-Mid-Devonian. They were then exhumed following the convergence of Avalonia and Laurentia and overlain with basal conglomerate and Great Scar Limestone during the Early-Carboniferous (Stone, Cooper, & Evans, 1999, pp. 330–3; Soper & Dunning, 2005, pp. 258–9; Barnes, Branney, Stone, & Woodcock, 2006, pp. 107–13; Waltham, 2008, pp. 14–29, 52–63; Stone et al., 2010, section 2). The Torbay unconformity formed during the later Variscan Orogeny when Upper-Devonian Old Red Sandstone originally deposited during the Caledonian Orogeny became folded as Euramerica and Gondwana converged in the Late-Carboniferous–Early-Permian. It was subsequently overlain by New Red Sandstone during the Permian (Warrington & Scrivener, 1990, p. 263; Edwards et al., 1997, p. 183; Leveridge & Hartley, 2006, pp. 225–50). More distant compressive effects of the Variscan Orogeny resulted in the gentler inclination of the secondary strata in some of the above unconformities in Northern England and Scotland (Waltham, 2008, pp. 61–2; Browne & Barclay, 2005, p. 182).

More detailed explanation of the foregoing processes is unfortunately beyond the scope of this paper. Hopefully, though, the above summary is enough to show that our current interpretations of these unconformities are markedly different from Hutton's. Hutton believed that the strata were uplifted and displaced by molten matter exerting a *vertical* force directly under them. According to our current theories, however, they were compressed, folded, accreted, and rotated by converging plates exerting a *horizontal* force. Constituent (7) was clearly responsible in part for Hutton's successful novel prediction concerning unconformities. Given, then, that the displacement and uplift of the strata are now thought to have resulted from radically different processes, the use of this constituent seems, at least *prima facie*, problematic for selective realism.

At this juncture, the realist might point to the role of heat in the movement of plates. Thermal convection in the mantle, the realist might argue, caused the plates to move, and the movement of plates caused the displacement of the strata. Heat, moreover, was involved at this latter stage in that the strata were buried and metamorphosed in the hot mantle. Subducting plates, too, heat up, dehydrate, and flux the surrounding mantle with volatiles, causing parts of it to melt, producing igneous intrusions and extrusions. This accounts for the greater consolidation and deformation of the



primary strata as well as the igneous veins – important parts of Hutton's prediction. Heat, then, the realist could suggest, was the working part of (7). That heat was exerting a specifically *vertical force directly under the strata* was idle.

This response can perhaps account for the primary strata being more deformed, consolidated, and veined than the secondary strata. That the strata were subjected to heat specifically from beneath might arguably be idle here. That they were subjected to heat (in combination with pressure) may be sufficient to make the prediction. To account for the *position* of the strata, though, it seems less promising, since heat plays a more distant and less significant role here. Convection currents rise in the mantle at mid-ocean ridges. Here, vertical convection is transferred into a horizontal vector, providing a lateral push to the rear of the diverging plates. This is “ridge-push”. Ridge-push, however, typically occurs thousands of miles away from the subduction zones where strata are folded and displaced. So it is significantly different from heat exerting a force directly beneath the strata. Perhaps more importantly, ridge-push is a relatively insignificant force in the movement of plates. The main force driving plate motion is “slab-pull”, a gravitational force resulting from the negative buoyancy of the lithosphere relative to the mantle at ocean trenches. Essentially, the edge of the plate sinks down into the mantle, pulling the rest of the plate along with it. The slab-pull force is thought to be around four times that of ridge-push (Kearey, Klepeis, & Vine, 2009, pp. 380–403). Thus, the main cause of displacement of strata is not heat but gravitation. So with respect to the part of the prediction concerned with the *position* of the strata, arguably the *central* part, (7) must be considered radically false, and therefore to present, again at least *prima facie*, a problem for selective realism.

A possible response for the realist with regard specifically to the *position* of the strata might be to suggest that, although Hutton's theory about the specific nature and direction of the forces responsible for the uplift and inclination of strata is not (even approximately) true, the weaker claim that *forces* were responsible for these processes is true. This part of (7), the realist could argue, was doing the work in deriving the prediction. The part concerning the particular nature and direction of the forces is idle.

A potential issue with this response is that it may be question-begging. As Stanford (2003, pp. 914–5; 2006, pp. 166–7) observes, a problem with selective realism is that the selective realist is apt simply to attribute responsibility for a given success to the constituents of the theory that have been retained in current theories. The problem here, of course, is that this is precisely the same criterion used to judge, for the purpose of the realism debate, whether or not a constituent is (approximately) true. By using a common criterion for attributing both responsibility and (approximate) truth, the (approximate) truth of responsible constituents is guaranteed, and so the strategy begs the question in favour of realism. It is not entirely clear that this is what the realist is doing here, however, for the realist could reply that she is distinguishing between (7) and the weaker claim that forces are involved not on the grounds that the latter is *true* but on the grounds that it (together with the other constituents involved) is sufficient to make the prediction, and so the criterion for attributing responsibility is in fact independent of whether the claim has been retained in current theories, and is therefore non-question-begging. What does seem clear is that whether this response is persuasive depends in large part on how we attribute predictive responsibility to theoretical constituents. If, following, for example, Psillos (1999, p. 105), we attribute responsibility only to those constituents which are *ineliminable* in deriving the prediction, then the response may have some purchase. If, on the other hand, we maintain with, for instance, Lyons (2006, pp. 543–4; 2009, pp. 150–1) that any constituent *actually used* by the scientist in the derivation of the

prediction should be considered responsible for the prediction's success, then the response will look less plausible.

Before concluding, a final, related point worth considering is that, depending on how we attribute predictive responsibility, it may turn out that constituents of Hutton's theory less obviously and less directly connected to the successful predictions but more obviously radically false were responsible, too. Consider Hutton's reasoning detailed above (pp. 5–6). The final cause of the earth (1) is to provide a habitable world. An efficient cause of its being habitable is its fertile soil. This requires the destruction of the land. The efficient cause therefore undermines the final cause. But given (2) that the earth is divinely contrived, it cannot simply be decaying such that it will become uninhabitable. So, there must necessarily exist some kind of natural process by which land is replenished such that the earth can maintain its purpose. To think about what this process consists in, we must think of the earth as being (4) analogous to an organic body. Organic bodies are reproduced via the same process which originally brought them into being. Therefore, land must be reproduced via the same process through which it was originally formed.

It was the above apparent conflict between final and efficient causes, what Gould (1987, p. 76) calls “the paradox of the soil”, that necessitated there being some process of restoring the land. Had Hutton not believed in (1) a final cause and (2) divine contrivance, then there would have been no paradox and thus no need to resolve the paradox by positing some kind of restorative process. When inquiring into what this process consists in, thinking of the earth as (4) analogous to an organic body was essential. For this gave Hutton his notion of cyclicity, the idea that future land must be produced via the same mechanism as the current land was formed. Without (1), (2), and (4) there would most likely have been no process of consolidation and uplift, and therefore no predictions. These constituents, then, seem in part responsible for Hutton's successes.

Lyons (2006, p. 543) has claimed that if we are to

take seriously ... the deployment [selective] realist's fundamental insight that credit should be attributed to those and only those constituents that were genuinely responsible, that actually led scientists to, specific predictions ..., [then] [c]redit will have to be attributed to all responsible constituents, including mere heuristics (such as mystical beliefs), weak analogies, mistaken calculations, logically invalid reasoning etc.

If this is correct, then (1), (2), and (4), insofar as they contributed to Hutton's predictions, are, by realist lights, confirmed by his successes. Indeed, if Lyons is correct, then other false posits used in Hutton's reasoning, such as the mistaken belief that there are only two possible causes of consolidation and the erroneous interpretation of metalliferous veins as igneous intrusions, get confirmed as well. Hutton certainly believed that (1) and (2) were confirmed by his successes. Following his description of the unconformities in Scotland, he wrote:

[b]y thus admitting a primary and secondary in the formation of our land, the present theory will be confirmed *in all its parts*. For nothing but those vicissitudes, in which the old is worn and destroyed, and the new land formed to supply its place, can explain that order which is to be perceived in all the works of nature; or give us any satisfactory idea with regard to that apparent disorder and confusion [i.e., the destruction of the land], which disgrace an agent possessed of wisdom and working with design (Hutton, 1795a, pp. 471–2 [emphasis added]).

And at the end of his second chapter on granitic veins, he asserted that his observations were proof of

a system in which the subterranean power of fire, or heat, co-operates with the action of water upon the surface of the Earth, for the restoration of that order of things which is necessarily lost in the maintaining of a living world – a world beautifully calculated for the growth of plants and nourishment of animals (Hutton, 1899, pp. 88–9).

The selective realist will surely not want to admit that Hutton's success warrants realist commitment to these patently false constituents. However, they did play a role in the theory's success. So the selective realist will need somehow to distinguish between the kind of role these constituents played and that played by other constituents. A possible way to do this, drawing on Vickers (2013, pp. 198–201), is to argue that these constituents played a *less direct* role than other constituents, and as such, although they influenced Hutton's thinking in certain ways, they were not really responsible for his success in a way that warrants realist commitment. They perhaps served as useful heuristics in formulating (3), (5), (6), (7), and (8). But it was these latter constituents that were directly responsible for the success and thus only these constituents that warrant any degree of realist commitment.

This strategy seems plausible insofar as the false constituents were indeed further back in the process of reasoning that led Hutton to his predictions. Indeed, it seems arguable that for just about any successful novel prediction, were we to trace the derivation back far enough, we would probably arrive at posits that were somehow connected with, or even causally indispensable for, the prediction, but which were not responsible for it in a way that warrants realist commitment to those posits. The challenge, however, is to produce a principled, non-ad hoc reason for only going back so far. We cannot simply stop just short of the point where we encounter a false posit, since this will beg the question in favour of realism. If such a principled reason can be given, then the realist might be able to rule these constituents out of being confirmed by Hutton's success. So far, no convincing, principled reason for only going back so far in the derivation of predictions has been proposed.

## 5. Conclusion

Authors involved in the realism debate (e.g., Lyons, 2002, p. 68; 2006, p. 557; Carman & Díez, 2015, p. 32) often talk as if *each individual* counterexample to the no-miracles argument is, by realist lights, a miracle. That is, they seem to interpret the argument as stating that *any individual instance of novel success* brought about by a false constituent of any given theory would constitute a miracle. A more charitable interpretation is that, given the *collective* success of our scientific theories, it would be a miracle if the responsible constituents were not *generally* at least approximately true. On such a reading, each individual counterexample does not by itself constitute (by realist lights) a miracle, but enough counterexamples, taken *collectively*, would. If this is correct, then settling the debate will involve analysing more potential counterexamples. If enough potential counterexamples cannot be accommodated by the realist, then selective realism will look less plausible. Conversely, if enough are put forward and the realist can accommodate them, then selective realism will look more plausible. Depending on the outcome, the antirealist or realist might then argue on inductive grounds that future apparent counterexamples will or will not be problematic. The task in hand, then, is clear. Historians and philosophers of science need to find and discuss

many potential counterexamples. The case of Hutton is one from an area of science which has been all but ignored in the realism literature, and one which, at the very least, deserves further attention.

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## References

- Barnes, R. P., Branney, M. J., Stone, P., & Woodcock, N. H. (2006). The Lakesman Terrane: The Lower Palaeozoic record of the deep marine Lakesman basin, a volcanic arc and foreland basin. In P. J. Brechley, & P. F. Rawson (Eds.), *The geology of England and Wales* (2nd ed.). (pp. 103–129). London: The Geological Society.
- Bowler, P. J. (2000). *The earth encompassed: A history of the environmental sciences*. New York, NY: Norton.
- Boyd, R. N. (1989). What realism implies and what it does not. *Dialectica*, 43, 5–29.
- Brown, J. R. (1982). The miracle of science. *The Philosophical Quarterly*, 32, 232–244.
- Browne, M. A. E., & Barclay, B. (2005). Siccar point to Hawk's heugh, Scottish borders. In W. J. Barclay, M. A. E. Browne, A. A. McMillan, E. A. Pickett, P. Stone, & P. R. Wilby (Eds.), *The Old Red Sandstone of Great Britain* (pp. 181–187). Peterborough: Joint Nature Conservation Committee.
- Carman, C., & Díez, J. (2015). Did Ptolemy make novel predictions? Launching Ptolemaic astronomy into the scientific realism debate. *Studies in History and Philosophy of Science Part A*, 52, 20–34.
- Carrier, M. (2004). Experimental success and the revelation of reality: The miracle argument for scientific realism. In M. Carrier, J. Roggenhofer, G. Küppers, & P. Blanchard (Eds.), *Knowledge and the world: Challenges beyond the science wars*. Heidelberg: Springer.
- Chew, D. M., & Strachan, R. A. (2014). The Laurentian Caledonides of Scotland and Ireland. In F. Corfu, D. Gasser, & D. M. Chew (Eds.), *New perspectives on the Caledonides of Scandinavia and related areas* (Vol. 390, pp. 45–92). Geological Society, London, Special Publications.
- Dean, D. R. (1992). *James Hutton and the history of geology*. Ithaca, NY: Cornell University Press.
- Deluc, J.-A. (1779). *Lettres physiques et morales, sur l'histoire de la terre et de l'homme* (Vol. 4). La Haye: Chez De Tune.
- Deluc, J.-A. (1780). *Lettres physiques et morales, sur l'histoire de la terre et de l'homme* (Vol. 2). La Haye: Chez De Tune.
- Earman, J. (1992). *Bayes or bust? A critical examination of Bayesian confirmation theory*. Cambridge, MA: MIT Press.
- Edwards, R. A., Warrington, G., Scrivener, R. C., Jones, N. S., Haslam, H. W., & Ault, L. (1997). The Exeter Group, south Devon, England: A contribution to the early post-Variscan stratigraphy of northwest Europe. *Geological Magazine*, 134, 177–197.
- Frost, B. R., & Frost, C. D. (2014). *Essentials of igneous and metamorphic petrology*. New York, NY: Cambridge University Press.
- Geikie, A. (1897). *The founders of geology*. London: MacMillan & Co.
- Gould, S. J. (1987). *Time's arrow, time's cycle: Myth and metaphor in the discovery of geological time*. Cambridge, MA: Harvard University Press.
- Hall, J. (1794). Sir James Hall on granite. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 3, 8–12.
- Hall, J. (1805). Experiments on whinstone and lava. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 5, 43–75.
- Hall, J. (1812). Account of a series of experiments, shewing the effects of compression in modifying the action of heat. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 6, 71–184.

- Hall, J. (1815). On the vertical position and convolutions of certain strata, and their relation with granite. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 7, 79–108.
- Harker, D. (2013). How to split a theory: Defending selective realism and convergence without proximity. *The British Journal for the Philosophy of Science*, 64, 79–106.
- Hesse, M. B. (1976). Truth and growth of knowledge. In F. Suppe, & P. D. Asquith (Eds.), *PSA 1976* (Vol. 2, pp. 261–280) East Lansing: Philosophy of Science Association.
- Hutton, J. (1785). *Abstract of a dissertation read in the Royal Society of Edinburgh, upon the seventh of March, and fourth of April, MDCCCLXXXV, concerning the system of the earth, its duration, and stability*. Edinburgh.
- Hutton, J. (1788). Theory of the earth; or an investigation of the laws observable in the composition, dissolution, and restoration of land upon the globe. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 1, 209–304.
- Hutton, J. (1794). Observations on granite. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 3, 77–85.
- Hutton, J. (1795a). *Theory of the earth, with proofs and illustrations, in four parts* (Vol. 1) Edinburgh: Cadell & Davies, London, & William Creech, Edinburgh.
- Hutton, J. (1795b). *Theory of the earth, with proofs and illustrations, in four parts* (Vol. 2) Edinburgh: Cadell & Davies, London, & William Creech, Edinburgh.
- Hutton, J. (1799). *Theory of the earth, with proofs and illustrations, in four parts* (Vol. 3) London: Geological Society.
- Jameson, R. (1808). *System of mineralogy* (Vol. 3) Edinburgh: William Blackwood.
- Jones, K., & Blake, S. (2003). *Mountain building in Scotland*. Milton Keynes: Open University Press.
- Kearey, P., Klepeis, K. A., & Vine, F. J. (2009). *Global tectonics* (3rd ed.). Oxford: Blackwell.
- Kitcher, P. (1993). *The advancement of science: Science without legend, objectivity without illusions*. Oxford: Oxford University Press.
- Kutilek, M., & Nielsen, D. R. (2015). *Soil: The skin of the planet earth*. Dordrecht: Springer.
- Laudan, L. (1981). A confutation of convergent realism. *Philosophy of Science*, 48, 19–48.
- Laudan, L. (1984). *Science and values*. Berkeley: University of California Press.
- Laudan, R. (1987). *From mineralogy to geology: The foundations of a science, 1650–1830*. Chicago: University of Chicago Press.
- Leggett, J. K., McKerrow, W. S., & Eales, M. H. (1979). The Southern Uplands of Scotland: A Lower Palaeozoic accretionary prism. *Journal of the Geological Society, London*, 136, 755–770.
- Leplin, J. (1997). *A novel defence of scientific realism*. Oxford: Oxford University Press.
- Leveridge, B. E., & Hartley, A. J. (2006). The Variscan Orogeny: The development and deformation of Devonian/Carboniferous basins in SW England and South Wales. In P. J. Brenchley, & P. F. Rawson (Eds.), *The geology of England and Wales* (2nd ed.). (pp. 225–255). London: The Geological Society.
- Lipton, P. (1990). Prediction and prejudice. *International Studies in the Philosophy of Science*, 4, 51–65.
- Lyell, C. (1830). *Principles of geology: Being an attempt to explain the former changes of the earth's surface, by reference to causes now in operation* (Vol. 1) London: John Murray.
- Lyons, T. D. (2002). Scientific realism and the pessimistic meta-modus tollens. In S. Clarke, & T. D. Lyons (Eds.), *Recent themes in the philosophy of science: Scientific realism and commonsense* (pp. 63–90). Dordrecht: Kluwer.
- Lyons, T. D. (2006). Scientific realism and the stratagema de divide et impera. *The British Journal for the Philosophy of Science*, 57, 537–560.
- Lyons, T. D. (2009). Criteria for attributing predictive responsibility in the scientific realism debate: Deployment, essentiality, belief, retention.... *Human Affairs*, 19, 138–152.
- Maxwell, G. (1962). On the ontological status of theoretical entities. In H. Feigl, & G. Maxwell (Eds.), *Scientific explanation, space, and time, Minnesota studies in the philosophy of science* (Vol. 3, pp. 181–192) Minneapolis, MN: University of Minnesota Press.
- McKerrow, W. S., Mac Niocaill, C., & Dewey, J. F. (2000). The Caledonian Orogeny redefined. *Journal of the Geological Society, London*, 157, 1149–1154.
- Miall, A. D. (2016). The valuation of unconformities. *Earth-Science Reviews*, 163, 22–71.
- Montgomery, K. (2003). Siccar Point and teaching the history of geology. *Journal of Geoscience Education*, 51, 500–505.
- Musgrave, A. (1988). The ultimate argument for scientific realism. In R. Nola (Ed.), *Relativism and realism in sciences* (pp. 229–251). Dordrecht: Kluwer.
- Oldroyd, D. R. (1980). Sir Archibald Geikie (1835–1924), geologist, romantic, aesthete, and historian of geology: The problem of Whig historiography of science. *Annals of Science*, 37, 441–462.
- Oldroyd, D. (1996). *Thinking about the earth: A history of ideas in geology*. Cambridge: Harvard University Press.
- Peters, D. (2014). What elements of successful scientific theories are the correct targets for “selective” scientific realism? *Philosophy of Science*, 81, 377–397.
- Playfair, J. (1802). *Illustrations of the Huttonian theory of the earth*. Edinburgh: Cadell & Davies, London, & William Creech, Edinburgh.
- Playfair, J. (1805). Biographical account of the late Dr. James Hutton. *Earth and Environmental Science Transactions of the Royal Society of Edinburgh*, 5, 39–99.
- Prothero, D. R., & Schwab, F. (2013). *Sedimentary geology: An introduction to sedimentary rocks and stratigraphy*. New York, NY: W. H. Freeman & Company.
- Psillos, S. (1994). A philosophical study of the transition from the caloric theory of heat to thermodynamics: Resisting the pessimistic meta-induction. *Studies In History and Philosophy of Science Part A*, 25, 159–190.
- Psillos, S. (1999). *Scientific realism: How science tracks truth*. London: Routledge.
- Putnam, H. (1975). *Mathematics, matter and method: Philosophical papers* (Vol. 1) Cambridge: Cambridge University Press.
- Ranalli, G. (2001). Experimental tectonics: From Sir James Hall to the present. *Journal of Geodynamics*, 32, 65–76.
- Rudwick, M. J. S. (2005). *Bursting the limits of time: The reconstruction of geohistory in the age of revolution*. Chicago, IL: University of Chicago Press.
- Rudwick, M. J. S. (2014). *Earth's deep history: How it was discovered and why it matters*. Chicago, IL: University of Chicago Press.
- Saatsi, J. (2012). Scientific realism and historical evidence: Shortcomings of the current state of debate. In H. de Regt, S. Hartmann, & S. Okasha (Eds.), *EPSA philosophy of Science: Amsterdam 2009. The european philosophy of science association proceedings* (Vol. 1, pp. 329–340) Dordrecht: Springer.
- de Saussure, H.-B. (1779). *Voyage dans les Alpes, précédé d'un essai sur l'histoire naturelles des environs de Geneve*. Neuchâtel: Chez Samuel Fauche.
- Smart, J. J. C. (1963). *Philosophy and scientific realism*. London: Routledge and Kegan Paul.
- Soper, N. J., & Dunning, F. W. (2005). Structure and sequence of the Ingleton Group, basement to the central Pennines of northern England. *Proceedings of the Yorkshire Geological Society*, 55, 241–261.
- Stanford, P. K. (2003). No refuge for realism: Selective confirmation and the history of science. *Philosophy of Science*, 70, 913–925.
- Stanford, P. K. (2006). *Exceeding our grasp: Science, history, and the problem of unconceived alternatives*. New York: Oxford University Press.
- Stephenson, D., & Gould, D. (1995). *Devonian, Grampian Highlands*. In: *British regional geology: Grampian Highlands* (4th ed.) Retrieved January 23, 2017, from [http://earthwise.bgs.ac.uk/index.php/Devonian\\_Grampian\\_Highlands#Lower\\_Old\\_Red\\_Sandston](http://earthwise.bgs.ac.uk/index.php/Devonian_Grampian_Highlands#Lower_Old_Red_Sandston).
- Stone, P. (2012). The demise of the Iapetus Ocean as recorded in the rocks of southern Scotland. *Open University Geological Society Journal*, 33, 29–36.
- Stone, P. (2014). The Southern Uplands Terrane in Scotland: A notional controversy revisited. *Scottish Journal of Geology*, 50, 97–123.
- Stone, P., Cooper, A. H., & Evans, J. A. (1999). The Skiddaw Group (English Lake District) reviewed: Early Palaeozoic sedimentation and tectonism at the northern margin of Avalonia. In N. H. Woodcock, D. G. Quirk, W. R. Fitches, & R. P. Barnes (Eds.), *In sight of the suture: The Palaeozoic geology of the Isle of Man in its Iapetus Ocean context* (Vol. 160, pp. 325–336) Geological Society, London, Special Publications.
- Stone, P., McMillan, A. A., Floyd, J. D., Barnes, R. P., & Phillips, E. R. (2012a). After Iapetus – Devonian-present, south of Scotland. In *British regional geology: South of Scotland* (4th ed.) Retrieved January 23, 2017, from [http://earthwise.bgs.ac.uk/index.php/After\\_Iapetus\\_%E2%80%93Devonian-present\\_South\\_of\\_Scotland](http://earthwise.bgs.ac.uk/index.php/After_Iapetus_%E2%80%93Devonian-present_South_of_Scotland).
- Stone, P., McMillan, A. A., Floyd, J. D., Barnes, R. P., & Phillips, E. R. (2012b). Southern Uplands accretionary complex. In *British regional geology: South of Scotland* (4th ed.) Retrieved October 3, 2017, from [http://earthwise.bgs.ac.uk/index.php/Southern\\_Uplands\\_accretionary\\_complex](http://earthwise.bgs.ac.uk/index.php/Southern_Uplands_accretionary_complex).
- Stone, P., & Merriman, R. J. (2004). Basin thermal history favours an accretionary origin for the Southern Uplands Terrane, Scottish Caledonides. *Journal of the Geological Society, London*, 161, 829–836.
- Stone, P., Millward, D., Young, B., Merritt, J. W., Clarke, S. M., McCormac, M., ... (2010). Acadian Orogeny, Devonian, northern England. In *British regional geology: Northern England* (5th ed.) Retrieved January 23, 2017, from [http://earthwise.bgs.ac.uk/index.php/Acadian\\_Orogeny\\_Devonian\\_Northern\\_England](http://earthwise.bgs.ac.uk/index.php/Acadian_Orogeny_Devonian_Northern_England).
- Vickers, P. (2012). Historical magic in old quantum theory? *European Journal for Philosophy of Science*, 2, 1–19.
- Vickers, P. (2013). A confrontation of convergent realism. *Philosophy of Science*, 80, 189–211.
- Waltham, T. (2008). *The Yorkshire Dales: Landscape and geology*. Marlborough: The Crowood Press.
- Warrington, G., & Scrivener, R. C. (1990). The Permian of Devon, England. *Review of Palaeobotany and Palynology*, 66, 263–272.
- Worrall, J. (1989). Structural realism: The best of both worlds? *Dialectica*, 43, 99–124.
- Young, D. A. (2003). *Mind over magma: The story of igneous petrology*. Princeton: Princeton University Press.
- Young, G. M., & Caldwell, W. G. E. (2009). A new look at an old unconformity: Field and geochemical data from James Hutton's original unconformity on the Isle of Arran, Scotland. *Proceedings of the Geologists' Association*, 120, 65–75.